The Behavior Analyst 1987, 10, 253–276 No. 2 (Fall)

Book Review

Drifting? Course? Destination?: A Review of *Research Methods in Applied Behavior Analysis: Issues and Advances*

Beatrice H. Barrett
Fernald State School

Quite befitting a young science, behavior analysis has engaged in considerable self-scrutiny in recent years. The list of scrutinizers continues to grow, as well it should. Not only is there much to be said, but, being accustomed to sparse and highly variable schedules, most critics do not expect immediate effects of their offerings. Yet, as the number of critical articles increases, there emerge common threads woven into the warp and woof of the basic premises of a natural science. The resulting pattern depicts an image of the current state of the field and how that state might be improved in the interest of advancing its core of verified facts, its methodology for expanding that foundation, and the means by which these advances might be applied to improve the human condition.

Like physiological medicine, behavior analysis, by the very nature of its subject matter, cannot escape its obligation to address problems of human health and welfare, especially as the latter continue to escalate with the pressures of an increasing population and consumerism that depletes the resources necessary to support a currently deteriorating quality of life. As human survival issues become more pressing, social "conscience" becomes more demanding of its technical community to produce cures for its ills, to somehow erase the effects of society's blundering efforts to cope with the effects of what it has wrought upon its environment. In society's innocence of how successful cures are born, it registers dissatisfaction with reputed therapies that fail to produce the results claimed to be imminent. Funds are diverted to other approaches, perhaps more familiar, with equally appealing claims, until they too fail to appease the increasingly impatient populace. And so on.

Behavior analysis, some twenty years ago, moved out of animal chambers into human chambers and, almost immediately, to the field of human behavior problems. The "flight from the laboratory" (Skinner, 1959) took many forms, chief among which were clinical and educational applications of reinforcement and punishment. Tackling some of the most hopeless and baffling cases, the new "behavior analysts" obtained more rapid and dramatic results than anyone had envisioned except, of course, the therapists themselves. Society was alerted. *Life* magazine in the mid-sixties published a front page story on the Lovaas autism "breakthrough." (No such publicity attended Ferster's seminal laboratory experiments with autistics, though they portended applications of programmed instruction to teach people with the most severe behavior deficits.) "Behavior modification" appeared in the titles of the first books on applied research and clinical applications of reinforcement and punishment principles (Krasner & Ullmann, 1965; Ullmann & Krasner, 1965). Agencies needed it for an avant garde

Research Methods in Applied Behavior Analysis: Issues and Advances was edited by Alan Poling and R. Wayne Fuqua and published by Plenum Press, New York, in 1986. My thanks to Gene Buchman for helpful discussion of certain technical aspects of the book. Requests for reprints should be sent to Beatrice H. Barrett, Psychology Department, Fernald School, Box 158, Belmont, MA 02178.

facade if nothing else. Clinicians sought it for their own reinforcement. The Kennedy administration made research and treatment of retarded *behavior* a cause celebre. Support for the new approach burgeoned. There was hardly a behavioral aberration that could not be fixed by the new methods. If they worked so well with severely disordered behavior, surely they could help control classroom misconduct, the asocial driving patterns of traffic offenders, the ecological and aesthetic destructiveness of littering, the morbid physiological results of overeating, etc. And they could even teach more acceptable behaviors to supplant those that had been eliminated.

Applications of behavior principles rode the crest of a new wave. Progress was everywhere and rapid. During an eight year period, three volumes appeared to announce new work on control of human behavior (Ulrich, Stachnik, & Mabry, 1966, 1970, 1974). Starting with basic principles, methods and assumptions, and illustrative applications to a wide range of behaviors, the first volume contained pioneering experiments that showed clearly how methods developed to analyze the behavior of small animals could control and thereby serve to analzye both normal and troublesome aspects of human behavior. Four years later, the second volume presented articles and research reports illustrating progress in applying the principles within a diverse spectrum of settings, dialogues dealing with efforts to control controversial, misunderstood, and often mislabelled "operant conditioning" procedures, and projections of a movement away from remediation toward development of preventive strategies. The final volume followed up on this course with emphasis on innovative applications in the field of education—the institution that could spawn the long-term commitment necessary for redesign of its methods and missions to prevent unintentional generation and maintenance of problematic behavior. Shortly thereafter two handbooks, one on research on behavioral treatment (Leitenberg, 1976) and the other on conceptual issues and applications (Catania & Brigham, 1978) documented the progress and status in a broad array of areas.

In short, these volumes presented issues and advances in applications of behavior analysis to the melioration of significant human behavior problems as well as projections for the future of the field in its impact on society. The period they represent was one of excitement and challenge, of dreams and hopes. The pace of innovative work quickened. Each new demonstration of effect seemed to imply that yet another domain would succumb to principles discovered in the laboratories of experimental behavior analysis. Each brought impetus for the next surge of application. What a bountiful voyage! The sleek-hulled beauty of operant conditioning, renamed "applied behavior analysis," gracefully sliced through the steep seas that had turned back many prior explorers. The wind of principle drove her onward, filling her sails with confidence, even arrogance, till her crew believed she could go anywhere and everywhere. After all, behavior abounded and society needed help to survive its own foibles. The current was favorable. Riding with it was exhilirating, energizing. New journals appeared at an accelerating rate. With all this activity we must have gone far on our way toward saving the world. But had we?

Skinner reminded us that problems of world magnitude remained unsolved (1982, 1986a), that a plethora of unanswered questions remained, that cognitive inroads were occurring. Ferster (1978) wondered "Is Operant Conditioning Getting Bored with Behavior?" Pennypacker (1986) admonished us about "buying in without selling out." And a host of concerned crew suggested we might be approaching some shoals. Had the current turned? Was the wind direction shifting? Had our course changed? Was our compass working? Where's the chart? In our rollicking ride, had we left it ashore? Our navigation text (Sidman, 1960) was out of print. We had scoffed at electronic aids. Who needs such in-

strumentation when you "know" the waters? But do we? What is our line of position? Our course made good? How could we determine it?

By deduction of course. But from what? Where is that newer navigation text (Johnston & Pennypacker, 1980)? The old salts could interpret and interpolate and perhaps help us avoid embarrassment, even disaster. But alas, we had buried it in a duffle and no one could remember its message. The crew was divided. Some thought we were doing just fine. Continue on present course (Baer, 1981). Others bemoaned our naivete and suggested we change course to locate an established navigation aid (e.g., Branch & Malagodie, 1980; Michael, 1980, 1985; Pierce & Epling, 1980). Our rotating skippers seemed unconcerned. There really are no shoals in this region. We're enjoying the sail, slipping through the water even faster! But have we moved much? Have we only the illusion of forward motion? Stalled or even backslipping in the now opposing current? Are we really approaching our destination? If so, what is it?

We might expect that a volume entitled *Research Methods in Applied Behavior Analysis: Issues and Advances* would address navigation problems in the field, where it is headed, forces that are impinging on its progress, new methodologic and conceptual contributions that might advance both its body of knowledge and the skills of its researchers in approaching this lofty objective. In addition, we might expect such a book to address some of the issues already extant in the literature. Such a volume, then, would depict the current state of the field's exemplary research methodology including the areas it is working on, suggested mechanisms for improvement, and advances that, if followed up, might accelerate progress toward what many would like to envision as a more mature science of human behavior.

There are such elements in its contents. The editors' preface indicates they have selected chapters to provide a summary of research methods and their application, and a "balanced coverage of controversial topics" without any attempt at resolution. Since the chapters are not organized under section headings or in any readily discernible sequence, I have taken the liberty of imposing a topical outline for purpose of discussing the state of the art, problems it is encountering, corrective mechanisms and, finally, issues of advancement.

STATE OF THE ART

A number of chapters reflect the current status of applied behavior analysis research methods. Some, in addition, contribute to other areas of content. The categories of topics offered here, though not mutually exclusive, may differ from those of other readers, depending on how their own conditioning histories influence their interpretations of what constitutes status, issues, and advances.

Flight to the Laboratory?

The book begins with Malott's chapter "Experimentation in Behavioral Psychology." Its subtitle, "Flight to the Laboratory," suggests that perhaps applied behavior analysis has rediscovered the laboratory, which the remainder of the book belies. Apparently addressing faculty colleague trainers of applied behavior analysts, Malott's is a plea for the field to loosen its constrictive overemphasis on the experimenter-scientist model, to encourage students to observe the world around them while speculating on the relevance of behavior analysis to everyday life and thereby to enrich the flora and fauna of their understanding of behavior. Malott fears that the quest for hard data and the requirements of an experimental approach will produce an "ever tightening spiral of triviality" (p. 5) at the expense of a broader world view. To counteract such influences, we should follow the leads of Darwin, Skinner, and even Dale Carnegie whose contributions he sees as deriving not from experimentation but from extrapolations and metatheorizing on a grand scale. While pursuing experimental training, we should

guard against indoctrinating our students to reject the ideas of their less scientific colleagues in the service field, for after all that is the arena in which most graduate trainees will eventually work. In keeping with the demands of the marketplace, we should face the issue of training people who will later become not researchers, but practitioners and administrators. The field of applied behavior analysis could benefit from admitting other sources of reinforcement.

Though on first reading a curious introductory chapter for a book on research methods, Malott subtly presages a great deal of what follows. While some might contest equating a descriptive biological scientist with an experimental behavior analyst with an interpreter of common sense, Malott's exhortation is surely valid advice for instructors, practitioners, administrators, and researchers alike. However, rather than viewing such activity in apparent opposition to experimental pursuits ("the blinders [of] experimental rhetoric"), astute observation of extra-laboratory phenomena might just as easily serve to generate models that simulate peculiarly human aspects of behavior for study under more controlled conditions, thereby expanding the experimental methodology applicable to increasingly relevant analysis of human behavior problems (Epling & Pierce, 1986; Epstein, 1986; Hake, 1982; Neuringer, 1984). The relation of experimental methodology and extra-laboratory observation and interpretation might ideally be one of symbiotic mutual enhancement.

Menu of Research Designs

Poling and Grossett's chapter, "Basic Research Designs," geared to a very introductory readership, presents a thumbnail sketch of the most prevalent research designs in the applied behavior analysis research literature along with some basic advantages and disadvantages of each. Included is a cursory injection of group comparison designs and use of inferential statistics to evaluate treatment outcomes with the caution that "between subjects designs in applied behavior analysis should be restricted to situations in which the experimental question necessitates their use" (p. 8). An in-press article appears as a sole reference for this statement without further qualification or clarification, leaving the reader unaware of the classic treatises on group designs (Bernard, 1865/1957; Johnston & Pennypacker, 1980; Sidman, 1960) and formulation of experimental questions (Johnston & Pennypacker, 1980), or even those relevant chapters appearing contemporaneously in the same volume (Johnston & Pennypacker, 1986a, 1986b). Only the last brief section on selecting a design offers an outline of criteria for choice from the buffet, then tells the reader to look elsewhere for the interstices. Here at last are a few of the references one might have expected much earlier.

It is noteworthy that, for the most part, the data presented as exemplars demonstrate effects of treatment "packages" rather than analysis of variables that might affect the efficacy of the package or the generality of the obtained effects. The authors skirt this seminal consideration with their own judgment, "surely most of the interventions devised by applied behavior analysts (and demonstrated effective via within-subject designs) have proven widely effective," (p. 24) and reference to Sidman's (1960) discussion of mature experimenter judgment in evaluating data. Again a loose end dangles leaving the introductory reader to wonder how one acquires the stature of maturity that endows one with judgmental license. Such sagacity is not likely to accrue through the literature of demonstration studies that characterizes the current state of applied behavior analysis research.

One's concept of appropriate approaches to experimental design reflects one's concept of behavior and the evolving stages in its experimental analysis and practical application. Is behavior static enough to be fully represented by a snapshot? Or even a time-based series of snapshots? Or does it occur as dynamic ebb and flow—periodic or aperiodic? Do we have enough information on any re-

sponse class in human behaving to chart its fluctuating flow under different environmental conditions? If we are confident that we do, then we should be able to predict fairly well how that response class will respond when we alter certain of the environmental conditions that are temporally associated with its changes. If so, we have attainted the degree of experimental sophistication about that response class as a behavior-environment relation that substantiates what Sidman refers to as "mature experimental judgment." We acquire it through a rich history of online observation and interaction with the ongoing recorded behavior of our experimental subjects via the alterations we introduce in their immediate environment.

The dynamics of behaving will vary depending on the nature of the response class(es) we choose and a host of other variables, many unknown until we provide opportunity for them to emerge in recorded data. Before we ask a question of ongoing behaving, we should have familiarized ourselves with its periodicity, the magnitude and duration of its fluctuations, and the conditions that may be associated with them—factors that might shed light on what maintains the patterns we observe—factors we may wish to minimize or maximize in the process of adjusting the power of our behaviorscope. We may need to redefine the response class(es) to a more molecular level in order to observe the full range of variability we wish to study. There may be good reasons to alter the scheduling and duration of our observation/recording periods and to fine tune our transducing system to whatever features yield as rich a picture as possible of the phenomenon we seek to study. Only when we are receiving as much as possible of its range of naturally occurring characteristics and have isolated or controlled as many as possible of the factors influencing it are we ready to experiment with some confidence that whatever change may be associated with our intervention cannot be attributed to some other source of variability.

The intimacy of this pre-experimental discovery phase and the powerful mutuality of experimenter-subject relation that can be engendered during the fine tuning of a behaviorscope constitute the initial and perhaps most compelling features of eventual experimental design—the sort of playfulness that Ferster (1978) alluded to in his reminiscences of an earlier era—the interplay that continues as questions are asked by modifying parameters of the interacting environment and as answers emerge in subleties of altered patterns of recorded behaving. Flexibility in response to the organism's behaving and constant vigilance for lawful patterns are the prerequisites. This is the essence of Skinner's "Case Study . . ." (1959), the essence of Claude Bernard's physiological science (1865/1957), the essence of—yes—our own personal progress toward those jewels of "a-haas" when our more casual observations of nature's kaleidoscope reveal the orderliness recognized for centuries by others but rediscovered afresh by us. These are the hopefully recapturable subject-experimenter interactions that could inject new spirit into the quest for a viable science of human behavior melioration.

How does one grasp these quintessentials from a pre-set menu? Experimental design is a process, not a set of rules and recipes. What are the elements as they apply to the process? They have been teased out and explicated as separate yet interrelated components, each of which plays an integral role in determining the product referred to as experimental design: formulating experimental questions, defining relevant response classes, selecting appropriate dimensions of these behavior units, designing an accurate, sensitive, and stable transducing system that includes response detection and recording, applying appropriate units of measurement to the recorded products, selecting and designing experimental operations that address the question, and, finally, arranging their temporal sequence (Johnston & Pennypacker, 1980). Only the latter appears in this chapter's treatment of single subject research designs.

Is this the experimental rigor that might

entice or lull the field into a "spiral of triviality?" Independent variables—treatment packages—apparently are givens, not to be questioned, only to be evaluated as they may relate to some degree of change in some aspect of behaving. The purpose of design, then, is to evaluate a cluster of procedures rather than to analyze either the package or the behavior conglomerate it is meant to affect. In this portrayal representing the current status of applied behavior analysis research, neither experimental development of treatments nor analysis of dependent variables are addressed. Nor are the problems of response class definition, observation, recording or measurement, all of which are endemic to experimental design in the field of behavior analysis. Rather, a potpourri of models and rules for their use seems to characterize the methodology of applied behavior analysis research. The exemplars presented seem to bear out the findings (Hayes, Rincover, & Solnick, 1980) and the contentions (Birnbrauer, 1979; Deitz, 1978; Michael, 1980, 1985; Pierce & Epling, 1980) that applied behavior analysis has drifted from its original conception (Baer, Wolf, & Risley, 1968). The contents of this chapter illustrate clear and pervasive departure.

Limitations on Generality

Other state-of-the-art chapters further confirm the departure. Fuqua and Bachman's chapter presents the empirical support of a survey of articles from the *Journal of Applied Behavior Analysis* during the period 1968 through 1982 with respect to the presence of five categories of information considered likely to affect reproducibility of results: demographic characteristics of subjects, diagnostic-descriptive labels or test scores, descriptions of relevant behavior repertoires including prerequisite behaviors, subject histories with the independent variable, and description of experimental conditions that include presence of contingencies in effect prior to introduction of the independent variable and information on reliability of treatment implementation. Other than age, gender, and diagnostic label or test score (included in, respectively, 88%, 63%, and 35% of the articles), the remaining categories are described in only 17% (S's history with the IV) to 38% (contingencies on DV during baseline) of the surveyed publications. Only 20% include information on prerequisite behaviors necessary for treatment implementation or information on reliability of procedural operations.

Similar surveys of information on subject selection criteria (Homer, Peterson, & Wonderlich, 1983) and reliability of treatment administration (Peterson, Homer, & Wonderlich, 1982), as well as trend analyses of idiosyncratic measurement units in the same journal (Barrett, 1983), further strengthen the conclusion that research in what is considered the most methodologically rigorous of the applied behavior analysis journals may not lend itself to replication simply because of incomplete procedural description. Thus, concordance of positive conclusions with respect to a given treatment cannot be taken as support for its generality.

The published research in a field conventionally presents the best of its work and serves as a vehicle for assessing its content, its direction, and its scientific stature. The data presented in this chapter empirically demonstrate the quality of at least some editorial review criteria for publishable reports. Since it seems clear that the field has embarked on a mission of technologic evaluation rather than analysis, it becomes all the more important that descriptions of method be both clear and complete, for the test of generality (seen by the authors as synonymous with applicability) will be through replication. Without adequate descriptive information, even that is impossible.

The criteria presented by Fuqua and Bachman could constitute a first approximation of a comprehensive checklist for use by both authors and editorial reviewers. Other entries on the guideline list should be drawn from Johnston and Pennypacker's (1980) analysis of adequate response class definition, observing, re-

cording, and measuring, for which there is available a checklist of major criteria (Barrett, Johnston, & Pennypacker, 1986).

With so little relevant research on which to base prognostically favorable selection of treatment, there is all the more need for detailed description of every aspect of procedure. This is especially true in a field that apparently eschews procedural standardization (see Perone, Galizio, & Baron, in press), even as an ideal. Application of its products will be performed primarily by sparsely supervised service personnel whose sophistication may be at best no more than that of the average lay person. Therefore, the directives to such consumers must be clear, concise, and simply stated. Task analysis could be put to beneficial use in this endeavor. Perhaps additions to the above-mentioned checklist could be derived from empirical studies of those descriptive modifications to selected published articles that are both necessary and sufficient to ensure accurate and reliable administration of selected treatment procedures by nonbehaviorally trained "average" service personnel. These criteria might ensure that socially valid studies could be useful in practice.

Observers as Psychometric Instruments

Response class definitions that fail to include the behavior property of "detectable displacement in space through time" (Johnston & Pennypacker, 1980, p. 48) force researchers to resort to human observing and recording rather than benefit from the direct (automatic) recording technology that abounds in everyday life. In applied behavior analysis research, reliance on human transduction has become so legion that even if response class definition is sufficiently discrete and unambiguous to permit automatic transduction, it is not used. As a result, the problems of observer accuracy and the variables of bias, drift, fatigue, and conditioning history must be dealt with in an effort to meet at least the requirement of objectivity traditionally associated with experimental inquiry.

Page and Iwata's chapter on the history, theory, and methods of assessing interobserver agreement begins with the premise that "research in applied behavior analysis involves the measurement of behavior under conditions precluding use of precision . . . recording equipment often found in experimental laboratories" (p. 99). Studying diverse response classes and processes in a wide range of settings prevents direct recording. In the age of robotics, the validity of this argument to perpetuate traditional, highly problematic observation, recording, and measurement methods is questionable, especially when such homely gadgets as batteries, remote switches, and counters could go a long way toward solving this problem. A button-cell battery, adequate to power many sophisticated and inexpensive modern recording devices, poses no encumbrance that would limit field use. The authors, however, assert that "reporting indexes of observer agreement is now a *necessary component* [italics added] of applied behavior analysis research" (p. 100), implying that use of automatic transducers of any sort or even a single well-trained and well-calibrated observer is no longer acceptable in published reports.

The rest of the story consists of a brief history of psychometric reliability and various ways of calculating "reliability" indexes and of evaluating chance agreement between multiple observers. The historic perspective recounts the various methods of assessing reliability of psychometric tests, likening the agreement of two observers to both alternate test forms and test-retest reliability. Other standard psychometric terms such as objectivity, validity, and accuracy are defined, and readers are referred to other sources for elaboration of the latter two. Under the rubric of assessment techniques, is an excellent discussion of the weaknesses of correlation coefficient tests of observer agreement and of total, interval, and exact agreement formats, followed by a pitch for the superiority of the block-by-block format. However, there is no discussion of the well documented weaknesses of interval recording—the

format that introduced applied behavior analysis to the field of psychometrics. The issue of interobserver agreement is moot if data are forced into distorted forms by the recording procedure. Nor is there any treatment of the separate yet interactive contributions of response class definitions, observation techniques, recording techniques, and selection of measuring units. In fact, recording is equated with measuring (p. 103). Generally, observers only record; they usually do not apply measuring units to the recorded products of observation. A section on chance agreement presents a number of techniques for its quantification, but affords neither statistical derivation nor critique of their technical merit.

The thrust of this chapter is improved application of statistical tests without examining the assumptions implicit in their use. Contradiction occurs in discrepant statements regarding the relation of interobserver agreement and accuracy of recorded product (p. 100, p. 104). Sources of variability in observing and recording responses and methods of minimizing their effects are ignored. For treatment of this critical issue, the reader should be referred to its thorough treatment by Johnston and Pennypacker (1980).

By virtue of its omission, application of stimulus control technology to improve the observation and recording accuracy of human transducers appears to be rejected in deference to application of traditional psychometric assessment of observer "error." This approach implicitly treats the human observer as a psychometric instrument rather than as a behaving organism with observing and recording responses susceptible to training via methods of behavior analysis that need no recourse to the "vaganosis" of psychometry (Pennypacker, 1982).

The criteria of accuracy and stability of behavior transduction appear to have lost status in evaluating applied behavior analysis research methods. The dictates of a technologically oriented mission, as reflected in the research literature, seem to preclude efforts at improved definition, observation, and recording of the response classes deemed important to society. Current editorial criteria are said to mandate multiple observers by requiring statistical evaluation of interobserver agreement. Idiosyncratic coding, equally idiosyncratic observing and recording schedules, use of arbitrary, experimenter-determined time intervals as units of measurement, and statistical rather than stimulus control approaches to transducer accuracy and stability should be added to Fuqua and Bachman's list of factors limiting the generality of applied behavior analysis research.

The mere fact that researchers perform their studies in field settings is no justification for abandoning efforts to control as many as possible of the controllable variables that might obscure or confound treatment effects (Barrett et al., 1986). It is no justification for either response class definitions incompatible with a natural science concept of behavior or for methods of transducing those classes into measurable form that, themselves, add undetected variability. It is surely no feasible reason for introducing such nonstandard measurement units as observation intervals. All of these sources of variability are controllable. If remedied in a manner consistent with pursuit of a natural science of human behavior, each would increase the likelihood of procedural integrity in the replication process and, in so doing, would likely enhance the generality of effects. These components of the measuring process are ably handled by the methods of experimental behavior analysis, many features of which *can* readily be directly applied outside the laboratory.

While social criteria may be permitted some influence on selection (but not definition) of response classes for treatment, the procedures used to evaluate effects experimentally should not be allowed to fall prey to the dictates of expedience or convenience. The social gain may not exceed the scientific sacrifice. Social appeasement and scientific integrity need not be mutually exclusive. In the long run, applied behavior analysis research will offer greater service to society by refining and updating its parental meth-

odology through use of available state-of-the-art engineering technology than by promulgating prebehavioral methods.

Graphic Analysis

The chapter by Parsonson and Baer is a conceptually puzzling amalgam. It begins with reflection on the success of visual analysis of graphically recorded data in experimental behavior analysis and its adoption by researchers in the applied field. It then raises the question of whether this tradition should continue, citing insensitivity, competition from statistical analysis, low interjudge agreement on interpretation of graphic displays, and discrepancies between graphic and statistical analysis. Most of the chapter uses simulated data as a point of departure for discussing the visually striking aspects of graphically displayed data that should control experimenter behavior. A short section then closes the chapter by noting a few of the format characteristics (scale variables) and contextual characteristics (political) that may influence interpretation of graphically presented data. A final "apologia" contends that graphic analysis yields greater information and analytic power than statistical analysis and therefore, though not stated, should continue.

Why the question of statistical analysis was raised is not clear, though one of Huitema's "commandments" in another chapter suggests an answer. Since so much of what transpires in the field is politically influenced, that rationale is suspect. If it was a straw man, the statistical argument seems to have been dispatched early on. But having introduced it opens up a distracting bag of worms that the apologia fails to account for, most notable of which are the differing purposes of the two approaches and the differing notions of the functions and sources of variability associated with each. Similarly, characterizing graphic analysis as "insensitive" raises such issues as the size and range of the units depicted on both axes, the nature of measurement units applied to obtain plotted values, the observation and recording techniques used to extract measurable products from response *effects* and, finally, the composition and definitional integrity of the response classes chosen as dependent variables—in short, the major components of behavior measurement and graphic display format. The resolving power of the graphic format cannot improve the sensitivity of the procedures used to obtain the values it portrays. But these considerations pale in importance when the overriding concern is the "conservative" virtue of insensitivity in flushing out only big effects (i.e., those that will ultimately bring social approval) and doing so expeditiously without obtaining the control of baseline variability that increases sensitivity to small effects. Carefully calibrated procedures might burden investigators with discovering too much too soon. Those who choose to follow up small effects in search of potentiating variables may find themselves researching the control of human behavior more in the manner of a behavior analyst than that of a package evaluator, and the decision could incur political costs.

The major discussion of this chapter treats ongoing graphic analysis as a tool for design modification. Drawing on simulated graphs, visually salient features and their associated interpretations reveal a number of problems encountered when attempting to analyze the graphics in published reports. Most show little if any concern for obtaining steady states before changing conditions. Regarding an eight-point increasing baseline trend followed by initiation of training,

> . . . it does not really matter whether the baseline is truly increasing; what matters is that any intervention applied after this baseline must produce increases that contrast to this apparent trend. A researcher concerned with whether this baseline is truly increasing simply must collect and inspect more of it, without intervention. A researcher whose concern is only to evaluate the effectiveness of the intervention poised for imposition on this baseline might well decide to proceed after these eight points . . . (p. 167)

Another commonly encountered problem in the literature turns up in the authors' exemplars: using idiosyncratic discontinuous units (sessions) on the abscissa while implying temporal continuity by

connecting adjacent points (p. 167). This practice (now a convention) not only fails to provide a true picture of the course of treatment, but implies that the likely unequal time intervals between sessions play no role in the obtained variability when, chances are, the question has not even been raised. Temporal distortion in graphic display formats (another manifestation of insensitivity) is not discussed (but see Huitema's chapter 10), nor is the influence of display tactics on interpretive behavior (see Johnston & Pennypacker, 1980), nor is the standard celeration chart (Pennypacker, Koenig, & Lindsley, 1972), one of the most useful tools for graphic analysis of trends in behavior change.

There is a good deal of very useful information for the beginner in the "fine-grained graphic analysis" section, but the reader should be aware that experimental design, measurement, graphic display, and interpretation, while not distinguished in this chapter, are separable and distinct investigator behaviors, each serving its own functions, each with its own sources of stimulus control (Johnston & Pennypacker, 1980). Supplemental reading to clarify these issues is a must.

Statistical Analysis: Misunderstanding, Rules, and Cures

Huitema's lucid chapters on autocorrelation and his commandments for the timely and appropriate use and interpretation of inferential statistics are efforts to dispel misconceptions and to correct faulty analyses found in the literature. These chapters should be thoroughly digested by those who have followed or who wish to catch up on this much-debated topic. The author reviews basic points and presents errors of both reasoning and computation in previous literature recommending time-series analysis as replacement for analysis of variance when its residuals are autocorrelated. He then surveys the "statistical structure" of data sets in the *Journal of Applied Behavior Analysis* from 1968 to 1977 (correlation coefficient for interobserver agreement!). Of 881 data sets, 440 were excluded because fewer than six baseline observations were obtained. Modal number was 3 to 4 baseline points. (The number of observations in subsequent phases was smaller.) The remaining 441 sets, comprised of 1,748 phases, yielded an autocorrelation coefficient of nearly zero. The general paucity of points and absence of equal time intervals in observing/recording schedules disqualify most applied behavior analysis data as time series, thereby clearing the way for analysis of variance of means across sequential phases—if that is what one chooses.

Aside from its intended purpose, the survey's distribution of baseline observations provides empirical support for expanding the checklist of generality limitations described earlier. In combination with abandonment of steady-state strategies (their response cost is too high), these data further confirm departure of the applied field from the methodology that gave rise to its success. In place of experimental control comes statistical filtration based on assumptions inconsistent with analysis of the sources of variability in human behavior.

Huitema's "commandments" chapter deals humorously and incisively with misunderstandings and misconceptions emanating from both sides of the statistics debate. Included are Parsonson and Baer's contention that visual analysis detects only large effects due to insensitivity and those authors' discounting small effects as unimportant or too overburdening to researchers. Regardless of one's persuasion, the points made here not only make good reading, they make sense of the contradictions in the published literature, especially those regarding time-series analysis and the relative merits of graphic versus statistical analysis. Huitema suggests that both visual and statistical analysis be performed for credibility ("Thou shalt not commit political suicide"), for greater social impact by easing communication with nonbehaviorally trained but statistically seduced people ("mellowspeak is buying our graduates a lot more cooperation than behaviorese"), and because "statistics make people feel good" (p. 229). Be that

as it may, palliatives are no substitute for long-term, high-generality positive effects. And the variables accounting for such treatment effects will not be teased out by bastardizing a natural science concept of behavior or by recommending that variability go unanalyzed and uncontrolled in the service of political gains.

PROBLEMS ENCOUNTERED

Compounding the foregoing portrayal of current status are other issues—ethical, sociopolitical, and conceptual—that combine to further cloud the atmosphere. These emanate from the context in which research takes place and the conditioning histories of those who conduct it. They are approached along a continuum from external givens to self-imposed adoption of community values and, finally, to conceptual-methodologic decisions propaedeutic to the usefulness of applied behavior analysis as a field of application for principles, methods, and findings of experimental behavior analysis.

Ethical Standards, Aversive Control and Fraud

Efforts to ensure human rights of research subjects continue to spawn a variety of regulations addressing the ethical standards governing acceptable practices. The chapter by Neef, Iwata, and Page reviews informed consent, risk-benefit ratios, and external review as standards for subject protection, then presents a trend survey (no inter-observer agreement!) of aversive control in behavioral studies, the ethical safeguards reported, the severity of the behavior treated, the prior use of "less restrictive alternatives," and others. Although a majority of deceleration studies used punishment procedures, they show no consistent decrease over the period 1968–1981, relatively few studies reported informed consent, and the number of studies reporting consent decreased in the more recent years. Few researchers quantified potential side effects. When aversive chemical, mechanical, or electrical stimulation was reported, prior use of alternative treatment appeared in 40% of the articles with *Journal of Applied Behavior Analysis* topping the list at 58%.

The overall pitch is for disclosure of consent to be required by editorial review boards, inclusion of data showing both collateral effects and follow-up, and research pertinent to the ethics of withholding aversive treatment pending failure of other treatment efforts. Unfortunately, the ethics of denying or terminating effective treatment due to disapproval by advocacy groups is not addressed.

In addition to the ethics of protecting human subjects, there are the ethics of scientific conduct. The chapter by Blakely, Poling, and Cross describes cases of apparent fraud throughout the history of science, offers speculations on why it occurs, and discusses some of the consequences along with measures that should assist in its prevention. The authors suggest that replication may be one safeguard. As indicated earlier, however, the published literature offers a myriad of obstacles to that endeavor.

The Methods and Promises of Social Validation

Closely related to externally imposed ethical standards are the internally imposed requirements of social validation as a means of ensuring research on changes of practical value in socially important behaviors—the major defining features of applied behavior analysis as described nearly twenty years ago (Baer et al., 1968). It would be well to remember that this defining feature, at that time, served to distinguish applied behavior analysis from experimental behavior analysis, its forerunner and source of both principles and methodology. Lever-pulling and button-pressing were considered convenient, not socially important, regardless of their functions as programmed in human operant laboratories. The leadership of applied behavior analysis made it abundantly clear that the new wave, at least as represented in the *Journal of Applied Behavior Analysis,* was not to be confused with such irrelevant

pursuits. Celebrating the first decade of that journal and the first volume of *Behavior Modification* came the dicta by which the social validity of research would be judged (Kazdin, 1977; Wolf, 1978). The objectives, procedures, and effects of a treatment procedure are submitted to consumers for subjective ratings of their importance. The procedures for social assessment of applied behavior analysis research are now said to be "standard" which, I presume, means a requisite for publication.

Fuqua and Schwade's thoughtful chapter addresses currently used social validation methods and evaluates the likelihood of achieving the objectives that formed the rationale for their imposition. *Throughout, the reader should be aware that the social validation requisite views empirical findings as completely subsidiary to the consumer's subjective judgment in determining treatment acceptability.*

The meat of the chapter describes the methods employed to determine acceptability of treatment goals, methods, and outcomes. Articles exemplifying their application for each purpose are reviewed and penetratingly critiqued on practical, methodologic, and conceptual grounds. The chapter concludes by recounting the reasons for social validation and questioning the usefulness of this approach for its intended purposes.

The final sentence expresses a summary caveat that could well be expanded into a chapter itself:

> . . . we would like to caution behavioral practitioners and researchers neither to abrogate their professional responsibility nor to underestimate the value of behavioral training by relying exclusively on social-validation methods for the selection of treatment objectives, evaluation of treatment acceptability, or assessment of clinical outcome. (p. 289)

The methods described here, while widely used in "behavioral assessment," are not only weak from a measurement perspective, but are borrowed from areas of psychology whose concepts and methods conflict with the basic assumptions of behavior analysis (Barrett et al., 1986). Consumer psychology and psychometrics accept rating scales and scored questionnaires as major tools in their "measurement" armamentarium. But I suspect most applied behavior analysts who use them are trained neither in their construction nor in their psychometric evaluation or underlying assumptions. That these procedures not only violate a natural science concept of behavior, but introduce undetectable sources of variability by virtue of the types of units used in their scales, has been amply discussed (Barrett et al., 1986; Johnston & Pennypacker, 1980, 1986a).

There are behavior-analytic paradigms for analyzing choice and preference, some of which the authors refer to. If applied behavior analysis considers assessment of social validity to be mandatory, it would do well to consult the available literature in behavior analysis on these topics and to measure the behavior of judges as precisely and objectively as it professes to measure that of its research subjects. Both should be conceived and defined as functional operants. The rating scale can be converted into a behavior-analytic preference paradigm. Perhaps doing so would convince some applied behavior analysts that the form of the response (likely to be button-pressing) is not as functionally irrelevant as social criteria would have them believe!

Encroachment of Quasi-Behavioral Methods

Applied behavior analysis is said to be "behavioral" (Baer et al., 1968). To some practitioners the term applies only to actions that society categorizes as disrupting, dangerous, aberrant, and so on. The form of the behavior, its frequency, its disturbing effects on others, and its resistance to conventional methods of control determine whether professional help will be sought. Other forms of human behaving such as the three Rs and "activities of daily living" and their broad array of prerequisites and components may fall outside this restricted conceptual domain. What the term "behavior" may connote today is so variable, even among "behaviorally" trained profes-

sionals, that it often bears scant resemblance to its original definition by Skinner (1938). As a result, a study or a body of literature may appear to be concerned with a phenomenon thought of as "behavior" when in fact that appearance is deceptive either because the concept of the subject matter has been misunderstood or because the methods applied and the data obtained are incongruent with the implied concept. Much of "behavioral assessment," though nearly identical with the definition of applied behavior analysis (cf. Nelson & Hayes, 1979), displays this sort of conceptual-methodologic inconsistency (Barrett et al., 1986).

Johnston and Pennypacker offer a "biologically and empirically functional definition" of behavior (p. 30), an extension of Skinner's, each feature of which demands certain methodologic requisites. Methods derived directly from this definition, and thus concordant with it, will offer fewer unrecognized sources of variability than those derived from other conceptual frameworks and therefore will yield increased reliability and generality of interpretation. "Pure" methodologic practices (i.e., those concordant with the defined subject matter) permit undistorted representation of all fundamental behavior properties. Practices that may start with observations of well defined behavior, but that subsequently include methods that render an incomplete or diluted representation of its basic properties, are referred to as "quasi-behavioral research methods." Veracity of behavior portrayal distinguishes the two. The recommended strategy is one of conceptual-methodologic concordance (Barrett et al., 1986; Johnston & Pennypacker, 1980).

In Johnston and Pennypacker's chapter, the authors have condensed their original full analysis of "pure" natural science methods and "quasi-behavioral" social science methods, including the functions and assumptions of statistical inference (1980). Using the natural science definition of behavior as a constant reference, they contrast the two methodologic approaches with respect to each component of the investigative process: defining the unit(s) of analysis (response classes), selecting both the dimensions to be quantified and the measuring units to be applied, devising procedures for both observing and recording occurrences of behavior units, designing the experiment, quantifying and displaying its results, and interpreting the findings. The uses and limitations of each approach accompany its description. The authors' strategy is to treat the experimental process as a series of interrelated experimenter behaviors. Tactical decisions regarding each component should be guided by and, essentially, be under the control of the *subject's* behavior, the nature of the question posed, and the inferences necessary to obtain an answer. Methodologic decisions discordant with definitional integrity are those that dilute or misrepresent the basic features of behavior. Therefore, neither convenience nor political expedience nor personal bias nor preconceptions are compatible with the methodologic criteria necessary to represent behavior fully. To be of long-term scientific or social utility, methods of applied behavior analysis research should be conceptually consistent with the professed domain of inquiry.

Johnston and Pennypacker's analysis of the decisions an investigator makes in fashioning research methods, the assumptions implicit in these choices, and the scope and accuracy of the resulting data is a major contribution to the field. Regarding the consequences of each sequential choice as a potential source of variability expressed not only in the obtained data, but also in the resulting interpretations, provides valuable guides for investigators wishing to maximize opportunities for observing lawful patterns of human behavior.

As a summary of problems encountered, one has only to apply the criteria of pure behavioral research to the state-of-the-art chapters described above to conclude that, whether by design, misunderstanding, or definitional drift, the field of applied behavior analysis, like the field of behavioral assessment, ap-

proaches its subject matter with quasi-behavioral research methods. The question is: What obstacles to its future advancement are implicit in this body of methodologic decisions?

PRESCRIPTIONS FOR IMPROVEMENT

The outline I have imposed forces certain chapters into dual roles. Some, however, are essentially prescriptive in content.

Self-Control and the Methodologic Resources of Buddhism

In keeping with Malott's sentiments, Brown's chapter addressing the failure of behavioral technology to facilitate utopian cultural design, suggests ancillary study of any discipline offering other approaches to understanding human behavior, regardless of its conceptual or methodologic divergence from current practices. He views behavioral community technology as limited by lack of countercontrols in the controlling relations between experimenter and subject. Experimental manipulations, treated as independent variables requiring no further analysis, fail to provide accountability to the subject. When the experimenter becomes a subject in the utopian community experiment, that individual's behavior becomes both independent and dependent variable, thus converting the experiment into one on self-control. The "self" must then become the subject matter of analysis, at which point Brown advocates departure from behavioristic refutation of the controlling and controlled "self." Buddhistic meditation techniques offer an array of behavioral exercises to increase objective observation of the covert and overt behaviors constituting the "self," thereby providing access for analysis of reciprocal and self-control. Synthesizing behaviorism and Buddhism could encourage formulation of more general principles of behavior by including analysis of covert behaviors.

Brown's view of behavioristic limitations could surely be argued on both methodologic (Johnston & Pennypacker, 1980; Sidman, 1960) and philosophic grounds (Day, 1983). The epidermis is not a restricting factor in defining response classes. Moreover, although other views of human behavior are certainly worthy of critical interpretive analysis, it would seem premature to seek recourse in prescientific approaches before refining our own contingency analysis of controlling variables in social contexts. Advocating flight to nonbehavioristic domains as a mechanism of liberation conveys not only a restrictive concept of what constitutes behaving at various levels of analysis (see Thompson & Zeiler, 1986), but an unfounded impression of experimental behavior-analytic methods as constrictive in the analysis of human behavior. There is ample freedom available to us. It is up to us to test its depths and breadth before we abandon ship.

Strengthen Clinical Psychopharmacology

Arguing that clinical psychopharmacology and applied behavior analysis should display similar research characteristics—that it, those articulated by Baer et al. (1968)—Poling and Cleary's chapter urges the field to include drugs in its evaluation pursuits. In view of the well-known drug-behavior interaction phenomena, single-subject designs are preferred along with direct (meaning observed or contemporaneous) recording of overt behavior rather than recall-recording of inferred inner states. Absent both of these methodologic imperatives, clinical psychopharmacologic research would be enhanced by contributions of applied behavior analysis. To do justice to drug evaluation, applied behavior analysts will have to "appreciate" the extended time dimensions necessary for complete analysis of drug effects. Because of society's need for information on the behavioral actions of certain drugs, these authors contend that the "promise of applied behavior analysis will not be fulfilled" (p. 309) if its researchers, equipped as they are with the needed methods, fail to provide it.

The authors' unelaborated caution regarding temporal dimensions of drug effects should be underscored. Its implications for designing and scheduling of appropriate behavior recording and measuring systems are substantial (Johnston et al., 1983; Lindsley, 1962). Multiple response classes, simultaneously recorded and independently sustained, will be necessary to show emerging interactive effects. "Pure" behavior-analytic methods, appropriately calibrated for transducing and measuring behavior and for sustaining multiple selected behaviors over long periods of time are ideal for investigating the latency of onset, duration, magnitude, and specificity of drug effects. Their unusual sensitivity for this purpose has long been recognized, and the mutual contributions of behavioral pharmacology and experimental behavior analysis are considerable (Dews, 1986). Application to examine effects of various drugs on psychotic behavior (Lindsley, 1960, 1962) and to monitor depth of surgical anesthesia (Lindsley, Hobika, & Etsten, 1961) was demonstrated more than twenty-five years ago. Multiple behavior channels, automatically transduced and simultaneously and continuously recorded, each sustained by different reinforcers available in the natural environment, can reveal effects on target behaviors as well as concurrent effects on "adjustive" behaviors. One such paradigm was sensitive enough to detect the effects of two aspirin as well as the progressive effects, following injection of a brachial nerve block, on three concurrent but independently sustained channels of reinforced free operant behavior (observed in Lindsley's laboratory, c. 1962). With today's more sophisticated sensors, these methods are applicable in field studies. Applied behavior analysts wishing to become involved in drug studies would find these references rich in methodologic information and a rewarding point of departure for refining techniques already demonstrated to be highly sensitive and accurate as human behavior monitoring systems.

Currently popular methods necessitating human observers and often cumbersome intermittent scheduling of transducer operation are not likely to provide the complete behavior portrayal demanded by this mission, especially if the findings from single-subject replications are to be later subjected to large-scale group evaluations (see Johnston, in submission). Moreover, given that recording of multiple response classes will be necessary to assess primary effects as well as positive and negative side effects, analysis of their *pretreatment variability* will require extended time periods prior to the onset of formal drug evaluation. Although the applied behavior analysis field could benefit from this methodologic fallout, behavior analysts seeking the political accolades of high publication frequency are ill advised to consider such an undertaking. Those whose reinforcement might accrue by advancing the methodology of applied behavior analysis to analyze (as distinct from demonstrate) complex effects of pharmaceutic agents on human behavior will find not only a full magazine programmed on a long but highly effective VI, but an opportunity to produce information of basic use not only to society but also to the science of human behavior.

Simulation of "Human" Performance

Epstein's engaging chapter discusses use of simulations when it is impossible to conduct properly controlled experimentation with human subjects. Rather than attribute complex human behaviors to internal agents and entities that defy behavior analysis, he suggests animal instead of computer simulations as a point of departure. Drawing on his own work, the author describes experiments analyzing the effects of detailed, carefully planned training histories on emergence of performances plausibly similar to "self-awareness," "symbolic communication," "insight," and spontaneous use of tools and memoranda by pigeons. Inner states appear superfluous in explaining development of behaviors thought to be peculiarly "human."

That this chapter appears in a book on applied behavior analysis research is in-

deed a tribute to the editors, if for no other reason than it offers a breath of creative relief from the litany of problems and pressures described earlier. But there are other reasons, among which is the excitement of science and the delight of discovery inherent in imaginative application of laboratory behavior-analytic methods to "uniquely human" phenomena and conveyed unabashedly by the investigator. Charlie Ferster is beaming! Are there others in the laboratory who could kindle such a spark?

Posing Productive Questions of Nature

Whatever reinforcers Nature may harbor, her menu may appear meager if the questions posed for inquiry are themselves limited. Johnston and Pennypacker's second chapter analyzes the nature of experimental questions and their formulation, sources of their control by, for example, graduate training, experimental literature, resources of the work facility, experimental contingencies and, of course, the scope of one's observing behavior. Concurring with Malott, the latter is a most necessary supplemental antidote for what may be the very restricted horizon available in current literature. As guides to subsequent design, analysis, and interpretation, experimental questions reflect the basic versus technologic directions of the field. The manner in which questions are formulated, then, is critical to the productivity and quality of subsequent research as well as its potential use in cross-germination between basic and technologic arenas. As the authors so wisely emphasize,

> Our research . . . is our very core. It largely defines the field both to ourselves and others. It is the foundation of our offerings to the culture, . . . our primary justification for our existence as a point of view and an identifiable research community. (p. 67)

Given the limited resources of behavior analysis, the issue of the consequences traceable to research questions merits careful consideration.

The second part of the chapter analyzes the functions of experimental questions in guiding (1) the construction and conduct of experimental procedures, including experimental strategy, choosing response classes and designing their measurement operations, and selecting meaningful independent variables; (2) the quantitative and graphic methods chosen for data analysis; and (3) the manner in which the data are interpreted with reference to the question. Revealing as they are of the methods emanating from them and their influence on the quality and completeness of the obtained data, the role of experimental questions in determining the growth of the science and its impact on everyday life deserves significant and searching scrutiny. Observing the world around us includes examining the traces we leave, the way we produced them, and the contribution they make to the progress of the field and the betterment of society.

> The past and present questions of a scientific discipline describe exactly where it has been and where it is in its evolution as a way of learning something about some portion of the contingencies of Nature. (p. 81)

What sorts of questions is applied behavior analysis posing to Nature? Are they about behavior? About principles? About conditions under which phenomena occur? About procedures? About control? How biasing are they with respect to the data they may dictate? What is the relative frequency of question asking to demonstration statements? A telling portrayal could emerge from an analysis of the purported reasons for performing applied behavior analysis research. Categories of questions or goals are available (Johnston & Pennypacker, 1980; Sidman, 1960) as possibilities for characterizing the current status and likely direction of the field. Just as our questions track our course and current position, so does their trend project our future destination. Cardinal query: To what extent are our questions restricting our range of vision by curtailing our opportunities to receive Nature's omnipresent messages? What are the consequences for advancement of the field?

ISSUES FOR ADVANCEMENT

Much of the critique offered above arises from the premise that applied be-

havior analysis exists, at least in part, to contribute to more complete understanding of human behavior. It also assumes that human behavior, as a biological phenomenon, exhibits natural order discoverable through the generally accepted approaches of natural science. In the course of its observations, the review raised questions about the priorities of applied behavior analysis researchers and what their published behavior samples bode for the future of the field. It approached the book as a selection of these samples purporting to present methodologic advances and issues.

What has been found is a well-written collection of often conflicting viewpoints without editorial guidelines or strategic perspective on how these offerings relate to one another or what they imply for the future of the field. The fledgling student might be thoroughly confused by the absence of a position or set of recommendations beyond "thou shalt nots." The same student may respond willingly and readily to social needs and political pressures as determinants of later professional/investigative behavior without realizing the consequences of such a decision, or even that there are other avenues to pursue. The more advanced student should evaluate the contents with respect to critiques and issues that have appeared repeatedly since the inception of *The Behavior Analyst,* as well as the issues addressed in our classic methodologic texts (Johnston & Pennypacker, 1980; Sidman, 1960). As supplemental reading, this volume provides ample points of departure for substantive critical discussion.

Analytic Versus Social Validation Priorities

Determining essential treatment elements. One of the most salient issues not addressed in this volume is the oft lamented and apparently increasing trend toward package evaluation in place of analytic research. What makes a package effective seems to be of little import when one's professional community dictates precedence of social over scientific validation and thus fails to support the necessary analysis. An offhand "who cares?" is more than mere irresponsibility. It is a telling symptom of the more general packaging mania that fills dumps with containers whose contents have been consumed with high-rated satisfaction, but whose lasting effects serve not to enrich but to destroy both the packager's and the consumer's supporting environment. If only the immediate function of the package is important, little reason exists to heed the suggestion that analysis of controlling variables in both pretreatment and treatment environments would reveal the conditions necessary for generality of effect and that such an endeavor would augment future applicability by assaying the essential and most effective variables and the principles governing their action. In the absence of this information, practitioners have no substantiated guidelines for selecting the most efficient treatment conditions associated with the most favorable prognosis.

Packages assembled via armchair speculation and circulated via nonanalytic demonstration are little more than documentation of effective practice. Proliferation of such demonstrations may temporarily appease society's pressing demands, but encouraging such "naked empiricism" (Birnbrauer, 1979) contributes little toward developing preventive strategies for long-term social benefit.

Portraying behavior and its functions. Other than a brief treatment by Johnston and Pennypacker (chapter 3), I am struck with the absence of concern with the putative subject matter—behavior—how to contact it to reveal its stable characteristics and how to portray those characteristics. The implication is that applied researchers are content with their current methods of defining, transducing, and measuring the phenomena that society deems worth fixing. As long as a consumer-validated big effect can be demonstrated, does it matter that there is no picture of how the target response classes function in the unaltered environment? Yet these are the functions that must be altered if treatment effects are to be sustained in the untreated setting. And does it matter that steady state methodology has been forsaken apparently because it is not convenient for the investigator or

the human transducers or the consumer to invest the necessary time? Yet the information deemed unimportant by this decision would shed light on variables influencing magnitude and reliability of treatment effects as well as the relevance of established animal behavior *patterns* for human behavior. These patterns often emerge only after hundreds of intermittently scheduled reinforcements necessitating prolonged exposure to programmed contingencies. If human behavior were provided such a methodologic privilege, would we obtain more reliable information than is currently available using the brief and infrequent sampling that characterizes applied research? Small wonder that statistical tests have regained such prominence.

How much more diluted will applied research become as it progressively divorces itself not only from its former professed conceptual allegiance to the study of behavior, but also from the natural science methods attendant to that approach? Treatment "tricks" may be appealing to practitioners called upon to supply behavioral bandages. But when challenged in courts and legislative hearings, how irrefutably can a research foundation support these tricks and how confidently can a practitioner state that the chosen treatment is the appropriate one for the presenting problem? How easily can one educate lay people on the various functions of different topographies and how this information is used to formulate treatment if one has never been sufficiently controlled by one's data to pose these questions of it? Yet legislative bodies are now filling their educational vacuum with the impassioned pleas of advocates whose content is more compelling than the power of our research-based facts. Social appeasement and scientific integrity are not mutually exclusive. The farther the field drifts from concepts and methods that could establish the relevance, timeliness, and certitude of behavioral intervention variables, the more likely will sociopolitical forces dictate a reactive versus an informative future course for applied behavior analysis.

Collaborative Interaction with the Analysis of Behavior

Most critiques of current status note the progressively widening schism between basic and applied work. The insularity of applied behavior analysis reflects itself not only in its technical versus principle-oriented-analytic content but also in its literature references where dwindling frequency of cross-referencing reveals lack of interaction with experimental behavior analysis (Poling et al., 1981). That this book fails to address the phenomenon confirms a degree of separation that cannot help but affect prospects for both areas (Epling & Pierce, 1986).

Relating unanalyzed omnibus independent variables to unanalyzed linguistically convenient topographies can be of little inspiration to the basic science. Nonetheless, some areas of basic research bear close review with an eye toward their conceptual and methodologic contribution to more profound understanding of specific behavior problems as well as general behavior processes indigenous to human social interaction. And there is a model of synergistic relation between basic and clinical research that should find new application in the quest for a more complete science of human behavior.

Clinically relevant basic research. First, the realm of *social relations* analysis should be particularly salient in teasing out the contingencies that develop and later sustain the problematic behaviors that occupy applied workers. Reviewing methods for analyzing two-person interactions in such phenomena as cooperation and competition (Cohen & Lindsley, 1964; Hake & Olvera, 1978; Hake & Vukelich, 1973; Lindsley, 1966) and the visual and auditory components of social reinforcement during communication (Lindsley, 1969; Nathan, Schneller, & Lindsley, 1964) provides analogues applicable to understand better the nature of human interaction and to then teach more adaptive social patterns. Definitions of the bidirectional interactive na-

ture of social reinforcement (Lindsley, 1963), social relations, and types of social stimuli and social control (Hyten & Burns, 1986) should go far toward alleviating one of the vexing problems in applied behavior analysis: topographic rather than functional definition of social behavior classes.

Methods for interaction analysis present useful models for fashioning experimental analogues to analyze the functions gleaned from systematic observation of naturally occurring contingencies in the field situation. In this endeavor, the applied researcher would benefit from in situ consultation with a basic researcher whose own analogues may be adoptable or adaptable to analyze the social relations functions of clinically relevant topographies. With an appropriate analogue, more controlled principle-oriented analyses of field observed contingencies might shed new light on how to conceptualize the kinds of environmental modifications necessary to change the functions of existing interactions and thus to ameliorate many presenting clinical problems. Principles operating in such analogue analyses, along with the operating analogue itself, might eventually be used to train habilitative staff in the functions of their interactions with given clients. Continuous online automatic recording would provide trainees direct observation of developing client behavior *patterns* in relation to specified *patterns* of their own. Redefining social behavior as bidirectional functional relations and devising mechanisms for their analysis would enrich the methodology of applied behavior analysis and bring its work into the mainstream of human behavior analysis.

Equivalence relations is another area that will bear much fruit in the world of application, both for analysis of increasingly broader social relations units and for improving instructional methodology. (This volume totally bypasses the latter.) Sidman's elegant treatise on emergent verbal classes (1986) and its integration of exemplary thematic research on stimulus class equivalents illustrates systematic synthetic expansion of a basic unit of analysis to encompass almost countless environmental elements that might function to control behavior. If dovetailed with analysis of social relations, the horizon of behavior analysis would seem limitless. Integrated work of this nature would surely break applied behavior analysis out of the "caricature" (Miller, 1983) and "conceptual poverty" status (Lee, 1987) now ascribed to it.

A third area of what many have considered to be "peculiarly human" behavior, particularly germane both to practitioners and to applied researchers using subjects with developmental disabilities, is that of *instructed performance.* Correspondence between verbal and nonverbal behavior in the applied domain and analyses of the interactions of verbal behavior and schedule effects in the laboratory are naturals to promote productive interactions between these domains. The research literature is beginning to escalate (e.g., Baron & Galizio, 1983; Catania, Matthews, & Shimoff, 1982; Harzem, Lowe, & Bagshaw, 1978; Lowe, 1979; Perone et al., in press; Weiner, 1983). Counfounding contingency-governed and rule-governed behavior could explain considerable unanalyzed variability and lack of generality in applied work. Useful dialogue on conceptual issues of response class definition has just begun (Matthews, Shimoff, & Catania, 1987; Stokes, Osnes, & Guevremont, 1987), and some clinical implications have been offered (Hayes, 1987).

While still virtually neglected in the literature of applied behavior analysis, two other areas of basic research are particularly relevant in understanding what clinicians observe to be contingency-resistant behaviors: *schedule-induced adjunctive behavior* and *concurrent schedule effects* predicted from Herrnstein's matching law (1961). The former are side effects of reinforcement schedules controlling other behaviors (see Epling & Pierce, 1983; Falk, 1971, 1984), and may be likened to some of the "iatrogenic" effects of unanalyzed and uncontrolled "habilitative" regimes (see Foster, 1978).

The latter are sustained by matching responding to relative rates of reinforcement—those programmed contingently and those "extraneously" available in the natural environment. The importance of the latter in unprogrammed habilitative environments has long been acknowledged. Possibilities for counteracting the effects of matching are found in the relation of extraneous reinforcement rates to those provided by programmed contingencies. Published cases analyzed with mathematical expressions of the matching law demonstrate its relevance, especially when programming reinforcement for an "incompatible" behavior (McDowell, 1981, 1982; Myerson & Hale, 1984a). Despite skepticism based on incongruities between laboratory and field settings (Fuqua, 1984), there is strong support for its systematic application (Epling & Pierce, 1983; Myerson & Hale, 1984b; Pierce & Epling, 1983). Applied behavior analysis can hardly afford to overlook such an invitation proffered by basic researchers.

Continuous programmed environments constitute a methodologic advance so uniquely relevant to applied work that its absence in this volume reflects the depth of the applied-basic schism and the constrictive effects of devotion to the limited domain of therapeutic changes. Begun in the 1960s to study functional units of existing complex individual behavior within a naturalistic laboratory setting (Findley, 1962, 1966) and expanded in recent years to investigate social phenomena among members of groups (summarized by Bernstein & Brady, 1986), continuously programmed environments provide for addition of nearly unlimited ecological texture and analysis of more complete and more naturalistic behavioral units while retaining the most salient characteristics of the operant "preparation" (Skinner, 1986b)—continuous, simultaneous automatic recording of multiple dependent variables, precisely specified and automatically programmed independent variables, sensitivity to effects within a controlled environment, applicability to steady state performances, and simultaneous analysis at both molar and molecular levels. Properly conceived, constructed, and focused, a resource of this nature could expand the horizon of applied work to restore its scientific viability by expanding its capabilities for investigating a broad range of human behavior phenomena including socially significant problems—how to synthesize them, how to analyze the variables that sustain and alter them, and how to program for maximal generality. Such an analytic environment has the potential of providing the most mutually enhancing common ground for collaborative contributions from both basic and applied fields in an integrated effort to expand the science of human behavior. I can think of no more fertile training ground for those who would create the next century's maximally effective behavior supportive environments.

Laboratory interface with clinical and basic science. Over 120 years ago Claude Bernard (1865/1957) wrote, "For a man of science there is no separate science of medicine or physiology, there is only a science of life" (p. 146). While behavior analysis eschews the medical model of disease entities, it might find productive options by examining more closely the basic science-clinical application relationship first enunciated by the father of experimental medicine for, with minor alterations, he expounded a unified concept of a science of behavior. Thus, modified for our purpose, the quote above might read: For a behavior scientist there is no separate science of application or basics, there is only a science of behavior. Similarly, ". . . medicine does not end in hospitals, as is often believed, but merely begins there (p. 147)" might read: Behavioral treatment does not end in clinics, schools, park departments, etc., but merely begins there.

Bernard elaborated the necessity of clinical observation for the investigator and a laboratory for the practitioner. Clinical practice and experimental investigation were seen as inseparable. And today's modern medical practice extends Bernard's concept to include clinical laboratories wherein the tests developed

from basic research are applied to diagnose clinical problems. Treatments, synthesized in the basic science laboratories, are then monitored in clinical laboratories. The clinical laboratory in medical practice has become the interface between the basic medical sciences and the practitioners of their findings. Continuous interactive communication within this broad domain is nowhere more obvious than in medical school-affiliated hospitals and clinics.

Behavior analysis could benefit greatly from clinical laboratories designed to incorporate modern technologic advances for behavior observation, recording, measurement, and online graphic and quantitative analysis along with the human analogues of basic science methods for analyzing at least the phenomena mentioned earlier. Such a venture might best be engineered conjointly by university-affiliated basic and applied workers, the latter drawn from experimentally based graduate training programs in applied behavior analysis or from other programs that provide considerable experience in human operant conditioning. The clinical arena from which such a facility might draw its referrals or subject pool might be a residential treatment program at least parts of which might consist of daily living and educational programs in a setting that supports systematic inquiry as a means of improving its services to consumers—willing and hopefully eager to invest in what should be bounteous return from the resources provided. In such a collaborative endeavor, applied behavior analysis could most readily integrate with and contribute to the science of behavior analysis (see Epling & Pierce, 1986), each cross-fertilizing the other in the "analytic spirit of the experimental method" (Bernard, 1857/1965, p. 207).

CONCLUSION

A complete science of behavior spans the methods, concepts, and interests of nonhuman behavior analysis through human behavior analysis and into development, testing, and evaluation of clinically valuable technologic advances. A truncated spectrum at either end will endanger the future of the field. To be true to its label (Pennypacker, 1981) and its avowed purpose (Baer et al., 1968), applied behavior analysis must revive a working relationship with its basic science, thereby to enrich the opportunities of both for future growth.

The currents have shifted since the early conception of applied behavior analysis. While finding its quasi-behavioral "heart" (Wolf, 1978), the field seems to have lost its behavior-analytic bearings. More sophisticated navigation and sail-trimming to the winds of new principles would help compensate for its drift. The future course and destination of applied behavior analysis will require concerted *teamwork* on the part of all who hope to benefit from its still embryonic potential.

REFERENCES

Baer, D. M. (1981). A flight of behavior analysis. *The Behavior Analyst, 4,* 85–91.

Baer, D. M., Wolf, M. M., & Risley, T. R. (1968). Some current dimensions of applied behavior analysis. *Journal of Applied Behavior Analysis, 1,* 91–97.

Baron, A., & Galizio, M. (1983). Instructional control of human operant behavior. *The Psychological Record, 33,* 495–520.

Barrett, B. H. (1983, May). *Growth patterns of measurement in applied behavior analysis.* Invited address presented at the meeting of the Association for Behavior Analysis, Milwaukee.

Barrett, B. H., Johnston, J. M., & Pennypacker, H. S. (1986). Behavior: Its units, dimensions, and measurement. In R. O. Nelson & S. C. Hayes (Eds.), *Conceptual foundations of behavioral assessment* (pp. 156–200). New York: Guilford Press.

Bernard, C. (1865/1957). *Introduction to the study of experimental medicine.* New York: Dover Publications.

Bernstein, D. J., & Brady, J. V. (1986). The utility of continuous programmed environments in the experimental analysis of human behavior. In H. W. Reese & L. J. Parrott (Eds.), *Behavior science: Philosophical, methodological, and empirical advances* (pp. 229–245). Hillsdale, NJ: Erlbaum.

Birnbrauer, J. S. (1979). Applied behavior analysis, service and the acquisition of knowledge. *The Behavior Analyst, 2*(1), 15–21.

Branch, M. N., & Malagodi, E. F. (1980). Where have all the behaviorists gone? *The Behavior Analyst, 3,* 31–38.

Catania, A. C., & Brigham, T. A. (Eds.). (1978). *Handbook of applied behavior analysis: Social and instructional processes.* New York: Irvington.

Catania, A. C., Matthews, B. A., & Shimoff, E. (1982). Instructed versus shaped human verbal behavior: Interactions with nonverbal responding. *Journal of the Experimental Analysis of Behavior, 38,* 233–248.

Cohen, D. J., & Lindsley, O. R. (1964). Catalysis of controlled leadership in cooperation by human stimulation. *Journal of Child Psychology and Psychiatry, 5,* 119–137.

Day, W. (1983). On the difference between radical and methodological behaviorism. *Behaviorism, 11,* 89–103.

Deitz, S. M. (1978). Current status of applied behavior analysis: Science versus technology. *American Psychologist, 33,* 805–814.

Dews, P. B. (1986). Pharmacological contributions to experimental analysis of behavior. In T. Thompson & M. D. Zeiler (Eds.), *Analysis and integration of behavioral units* (pp. 135–159). Hillsdale, NJ: Erlbaum.

Epling, W. F., & Pierce, W. D. (1983). Applied behavior analysis: New directions from the laboratory. *The Behavior Analyst, 6,* 27–37.

Epling, W. F., & Pierce, W. D. (1986). The basic importance of applied behavior analysis. *The Behavior Analyst, 9,* 89–99.

Epstein, R. (1986). Simulation research in the analysis of behavior. In A. Poling & R. W. Fuqua (Eds.), *Research methods in applied behavior analysis: Issues and advances* (pp. 127–155). New York: Plenum Press.

Falk, J. L. (1971). The nature and determinants of adjunctive behavior. *Physiology and Behavior, 6,* 577–588.

Falk, J. L. (1984). Excessive behavior and drug-taking: Environmental generation and self control. In P. L. Levison (Ed.), *Substance abuse, habitual behavior, and self-control* (pp. 81–123). Boulder: Westview Press.

Ferster, C. B. (1978). Is operant conditioning getting bored with behavior? A review of Honig and Staddon's *Handbook of Operant Behavior. Journal of the Experimental Analysis of Behavior, 29,* 347–349.

Findley, J. D. (1962). An experimental outline for building and exploring multi-operant behavior repertoires. *Journal of the Experimental Analysis of Behavior, 5,* 113–166.

Findley, J. D. (1966). Programmed environments for the experimental analysis of human behavior. In W. Honig (Ed.), *Operant behavior: Areas of research and application* (pp. 827–848). Englewood Cliffs, NJ: Prentice-Hall.

Foster, W. S. (1978). Adjunctive behavior: An under-reported phenomenon in applied behavior analysis? *Journal of Applied Behavior Analysis, 11,* 545–546.

Fuqua, R. W. (1984). Comments on the applied relevance of the matching law. *Journal of Applied Behavior Analysis, 17,* 381–386.

Hake, D. F. (1982). The basic-applied continuum and the possible evolution of human operant social and verbal research. *The Behavior Analyst, 5,* 21–28.

Hake, D. F., & Olvera, D. (1978). Cooperation, competition, and related social phenomena. In A. C. Catania & T. A. Brigham (Eds.), *Handbook of applied behavior analysis* (pp. 208–245). New York: Irvington.

Hake, D. F., & Vukelich, R. (1973). Analysis of the control exerted by a complex cooperative procedure. *Journal of the Experimental Analysis of Behavior, 19,* 3–16.

Harzem, P., Lowe, C. F., & Bagshaw, M. (1978). Verbal control in human operant behavior. *The Psychological Record, 28,* 405–423.

Hayes, S. C. (1987). Language and the incompatibility of evolutionary and psychological continuity. *Behavior Analysis, 22,* 49–54.

Hayes, S. C., Rincover, & Solnick, J. V. (1980). The technical drift of applied behavior analysis. *Journal of Applied Behavior Analysis, 13,* 275–285.

Herrnstein, R. J. (1961). Relative and absolute strength of response as a function of frequency of reinforcement. *Journal of the Experimental Analysis of Behavior, 4,* 267–272.

Homer, A. L., Peterson, L., & Wonderlich, S. A. (1983). Subject selection in applied behavior analysis. *The Behavior Analyst, 6,* 39–45.

Hyten, C., & Burns, R. (1986). Social relations and social behavior. In H. W. Reese & L. J. Parrott (Eds.), *Behavior science: Philosophical, methodological, and empirical advances* (pp. 163–183). Hillsdale, NJ: Erlbaum.

Johnston, J. M. (in submission). Comparing behavior change procedures. *Journal of Applied Behavior Analysis.*

Johnston, J. M., & Pennypacker, H. S. (1980). *Strategies and tactics of human behavioral research.* Hillsdale, NJ: Erlbaum.

Johnston, J. M., & Pennypacker, H. S. (1986a). Pure versus quasi-behavioral research. In A. Poling & R. W. Fuqua (Eds.), *Research methods in applied behavior analysis: Issues and advances* (pp. 29–54). New York: Plenum Press.

Johnston, J. M., & Pennypacker, H. S. (1986b). The nature and functions of experimental questions. In A. Poling & R. W. Fuqua (Eds.), *Research methods in applied behavior analysis: Issues and advances* (pp. 55–83). New York: Plenum Press.

Johnston, J. M., Wallen, A., Partin, J., Neu, E., Cade, R. F., Stein, G. H., Goldstein, M. K., Pennypacker, H. S., & Gfeller, E. (1983). Human operant laboratory measurement of the effects of chemical variables. *The Psychological Record, 33,* 457–472.

Kazdin, A. E. (1977). Assessing the clinical or applied importance of behavior change through social validation. *Behavior Modification, 1,* 427–452.

Krasner, L., & Ullman, L. P. (Eds.). (1965). *Research in behavior modification: New developments and implications.* New York: Holt, Rinehart and Winston.

Lee, V. L. (1987). Radical behaviorism and behavior analysis: A review of *Behaviour Analysis and Contemporary Psychology. The Behavior Analyst, 10,* 95–101.

Leitenberg, H. (Ed.). (1976). *Handbook of behavior modification and behavior therapy.* Englewood Cliffs, NJ: Prentice-Hall.

Lindsley, O. R. (1960). Characteristics of the behavior of chronic psychotics as revealed by free-operant conditioning methods. *Diseases of the Nervous System, 21*(monograph supplement), 66–78.

Lindsley, O. R. (1962). Operant conditioning techniques in the measurement of psychopharmacologic response. In J. H. Nodine & J. H. Moyer (Eds.), *Psychosomatic medicine: The first Hahnemann symposium* (pp. 373–383). Philadelphia: Lea & Febiger.

Lindsley, O. R. (1963). Experimental analysis of social reinforcement: Terms and methods. *American Journal of Orthopsychiatry, 33,* 624–633.

Lindsley, O. R. (1966). Experimental analysis of cooperation and competition. In T. Verhave (Ed.), *The experimental analysis of behavior* (pp. 470–501). Englewood Cliffs, NJ: Prentice-Hall.

Lindsley, O. R. (1969). Direct behavioral analysis of psychotherapy sessions by conjugately programmed closed-circuit television. *Psychotherapy: Theory, Research and Practice, 6,* 71–81.

Lindsley, O. R., Hobika, J. H., & Etsten, B. E. (1961). Operant behavior during anesthesia recovery: A continuous and objective method. *Anesthesiology, 22,* 937–946.

Lowe, C. F. (1979). Determinants of human behavior. In M. D. Zeiler & P. Harzem (Eds.), *Reinforcement and the organization of behavior* (Vol. 1, pp. 159–192). New York: Wiley.

Matthews, B. A., Shimoff, E., & Catania, A. C. (1987). Saying and doing: A contingency-space analysis. *Journal of Applied Behavior Analysis, 20,* 69–74.

McDowell, J. J. (1981). On the validity of Herrnstein's hyperbola in applied behavior analysis. In C. M. Bradshaw, E. Szabadi, & C. F. Lowe (Eds.), *Quantification of steady state behavior* (pp. 311–324). Elsevier/North Holland: Biomedical Press.

McDowell, J. J. (1982). The importance of Herrnstein's mathematical statement of the law of effect for behavior therapy. *American Psychologist, 37,* 771–779.

Michael, J. (1980). Flight from behavior analysis. *The Behavior Analyst, 3*(2), 1–21.

Michael, J. (1985). Fundamental research and behaviour modification. In C. F. Lowe, M. Richelle, D. E. Blackman, & C. M. Bradshaw (Eds.), *Behaviour analysis and contemporary psychology* (pp. 159–164). London: Erlbaum.

Miller, H. L., Jr. (1983). More than promissory? Reflections on the once and future experimental analysis of human behavior. *The Psychological Record, 33,* 551–564.

Myerson, J., & Hale, S. (1984a). Practical implications of the matching law. *Journal of Applied Behavior Analysis, 17,* 367–380.

Myerson, J., & Hale, S. (1984b). Concurrent schedules and matching in applied settings: A reply to Fuqua. *Journal of Applied Behavior Analysis, 17,* 387–389.

Nathan, P. E., Schneller, P., & Lindsley, O. R. (1964). Direct measurement of communication during psychiatric admission interviews. *Behaviour Research and Therapy, 2,* 49–57.

Nelson, R. O., & Hayes, S. C. (1979). Some current dimensions of behavioral assessment. *Behavioral Assessment, 1,* 1–16.

Neuringer, A. (1984). Melioration and self-experimentation. *Journal of the Experimental Analysis of Behavior, 42,* 397–406.

Pennypacker, H. S. (1981). On behavior analysis. *The Behavior Analyst, 4,* 159–161.

Pennypacker, H. S. (1986). The challenge of technology transfer: Buying in without selling out. *The Behavior Analyst, 9,* 147–156.

Pennypacker, H. S., Koenig, C. H., & Lindsley, O. R. (1972). *Handbook of the standard behavior chart.* Kansas City, KS: Precision Media.

Perone, M., Galizio, M., & Baron, A. (in press). The relevance of animal-based principles in the laboratory study of human operant conditioning. In G. C. L. Davey & C. Cullen (Eds.), *Human operant conditioning and behaviour modification.* Chichester, England: Wiley.

Peterson, L., Homer, A. L., & Wonderlich, S. A. (1982). The integrity of independent variables in behavior analysis. *Journal of Applied Behavior Analysis, 15,* 477–492.

Pierce, W. D., & Epling, W. F. (1980). What happened to analysis in applied behavior analysis? *The Behavior Analyst, 3*(1), 1–9.

Pierce, W. D., & Epling, W. F. (1983). Choice, matching and human behavior: A review of the literature. *The Behavior Analyst, 6,* 57–76.

Poling, A., Picker, M., Grossett, D., Hall-Johnson, E., & Holbrook, M. (1981). The schism between experimental and applied behavior analysis: Is it real and who cares? *The Behavior Analyst, 4,* 93–102.

Sidman, M. (1960). *Tactics of scientific research.* New York: Basic Books.

Sidman, M. (1986). Functional analysis of emergent verbal classes. In T. Thompson & M. D. Zeiler (Eds.), *Analysis and integration of behavioral units* (pp. 213–245). Hillsdale, NJ: Erlbaum.

Skinner, B. F. (1938). *The behavior of organisms.* Englewood Cliffs, NJ: Prentice-Hall.

Skinner, B. F. (1959). *Cumulative record.* New York: Appleton-Century-Crofts.

Skinner, B. F. (1982, August). Why we are not acting to save the world. Paper presented at the meeting of the American Psychological Association, Toronto.

Skinner, B. F. (1986a). What is wrong with daily life in the western world. *American Psychologist, 41,* 568–574.

Skinner, B. F. (1986b). Some thoughts about the future. *Journal of the Experimental Analysis of Behavior, 45,* 229–235.

Stokes, T. F., Osnes, P. G., & Guevremont, D. C. (1987). Saying and doing: A commentary on a contingency-space analysis. *Journal of Applied Behavior Analysis, 20,* 161–164.

Thompson, T., & Zeiler, M. D. (Eds.). (1986). *Analysis and integration of behavioral units.* Hillsdale, NJ: Erlbaum.

Ullman, L. P., & Krasner, L. (Eds.). (1965). *Case studies in behavior modification.* New York: Holt, Rinehart and Winston.

Ulrich, R., Stachnik, T., & Mabry, J. (Eds.). (1966). *Control of human behavior.* Glenview IL: Scott Foresman.

Ulrich, R., Stachnik, T., & Mabry, J. (Eds.). (1970). *Control of human behavior: Vol. 2. From cure to prevention.* Glenview, IL: Scott Foresman.

Ulrich, R., Stachnik, T., & Mabry, J. (Eds.). (1974). *Control of human behavior: Vol. 3. Behavior modification in education.* Glenview, IL: Scott Foresman.

Weiner, H. (1983). Some thoughts on discrepant human-animal performances under schedules of reinforcement. *The Psychological Record, 33,* 521–532.

Wolf, M. M. (1978). Social validity: The case for subjective measurement or how applied behavior analysis is finding its heart. *Journal of Applied Behavior Analysis, 11,* 203–214.